ORIGINAL PAPER

Cyclical behavior of hiring discrimination: evidence from repeated experiments in France

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Abstract

Our objective is to investigate if hiring discrimination in France has a cyclical nature using an innovative set of repeated correspondence tests. The methodology covers one type of job only, that of administrative manager, in both the private and public sectors, and two discrimination criteria, ethnic origin and place of residence. The empirical analysis is based on five waves of tests starting in 2015, covering the periods before, during, and after the first lockdown, with 4749 applications sent for 1583 job openings in total. Our results indicate that hiring discrimination based on the dual criteria of origin and place of residence has decreased in France since the mid-2010s, within the context of an improved labor market, but that it increased sharply during the Covid health crisis, in recessionary conditions, suggesting that it generally follows a counter-cyclical behavior. Overall, the temporal patterns of discrimination, as measured by callback rates, mirror those of the unemployment rate.

JEL Classification C81 · C93 · J15 · J45 · J71

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1 Introduction

The lockdown imposed to tackle the Covid-19 pandemic produced two types of major consequences for labor markets in most countries. Firstly, it brought about an unusually large reduction in working hours, which caused an unprecedented deterioration in the labor market and resulted in a sharp rise in unemployment (Belle and Blanchflower 2020; Mayhew and Anand 2020; Petrosky-Nadeau and Valetta 2020). Secondly, the shock had heterogeneous effects on different segments of the labor market depending on the sector, occupation, social group and territory concerned. In general, the most affected categories were those already most exposed to unemployment, especially ethno-racial minorities (Blundell et al. 2020; Fairlie et al. 2020).

Given the rise in both the overall level of unemployment and in the inequalities in its distribution, the issue of discrimination in access to employment is of crucial interest. When unemployment increases, recruiters find a lengthening queue of candidates applying for a shrinking number of jobs. To whittle this down, it can be tempting to actively consider personal characteristics unrelated to productivity in the selection process, which constitutes discrimination as defined by Heckman (1998). However, those who face hiring discrimination of this type are often also the people in jobs with the lowest pay, in particular women, people from ethnic minorities and people living in deprived neighborhoods. It follows that a rise in unemployment would go hand-in-hand with a rise in hiring discrimination, the effect of which would be to further increase the inequalities faced by those on the lowest incomes. Thus, the economic and social effects of the pandemic are expected to be aggravated by this rise in discrimination.

Our contribution is an investigation into the effects of the economic cycle on hiring discrimination in the labor market. The central hypothesis we are seeking to verify is that economic recession fuels discrimination in access to employment and, inversely, that economic recovery reduces discrimination. This has important implications in the context of the unprecedented pandemic, where the most vulnerable social groups are already the most affected by the health crisis, partly because they are more exposed to the virus due to their professional constraints (jobs involving exposure to customers, colleagues or patients in sectors particularly stressed by the health crisis; lack of remote-working opportunities and no recourse to partial unemployment) and partly because of their more vulnerable health (they are less likely to seek healthcare and have more pre-existing chronic health problems and co-morbidity factors such as obesity.) (Platt and Warwick 2020). If, indeed, recession creates circumstances conducive to discrimination, then these most vulnerable people are subject to consequences from two sources that are bound to fuel the processes of social exclusion.

To the best of our knowledge, few studies have tried to measure the evolution of discrimination over time, and even fewer have attempted to measure how recession impacts discrimination. Baert et al. (2015) confirmed the counter-cyclical hypothesis in a correspondence study of access to employment for members of the Turkish community in Belgium. Asali et al. (2018) found the same results

in the Georgian context while Carlson et al. (2018) invalidated it in the Swedish labor market. Dahl and Knepper's (2020) recent work focusing on age discrimination is also worth citing. It concludes that each percentage point increase in local unemployment reduces the callback rate for older women by 15% compared with younger women.

Specifically, we use the Covid-19 health crisis as an exogenous shock to detect the sensitivity of discrimination to recessionary pressures. We focus on two grounds for discrimination, ethnic origin and place of residence. Our objective is to measure how labor market discrimination evolves for French citizens of North African descent and residents of deprived neighborhoods in different economic circumstances. Ethnic origin is the most studied discrimination criteria with place of residence being less systematically considered in correspondence studies (Bertrand and Mullainathan 2004; Duguet et al. 2010; Tunstall et al. 2014; Bunel et al. 2016; L'Horty et al. 2019).

Our empirical analysis is based on data collected from five waves of correspondence tests conducted before, during, and after the lockdown in the eight *départements* of the Paris region in a relatively broad professional field with quite a high level of qualification required, that of administrative managers. In response to each job advertised in each *département*, we sent applications from three fictitious male applicants: a reference candidate of French origin living in a neutral neighborhood; a candidate whose name and surname indicate that he is of North African descent; and a candidate of French origin living in a deprived neighborhood. In France, these neighborhoods are officially recognized as Urban Policy Priority Neighborhoods (*Quartiers prioritaires de la Politique de la Ville,* QPV) and are zoned based on the level of poverty of their inhabitants.[1] Our dataset includes 1,583 positions tested over 5 years with 4,749 applications: 554 between October 2015 and March 2016, 384 between September 2017 and February 2018, 248 between October 2019 and February 2020, 194 between March and June of 2020 (during the lockdown period and immediately afterward), and 203 after the summer, in September and October 2020. The job offers tested are in both the private and public sectors. In France, a strong commitment to the republican principle of equality is associated with the public sector, although, on the other hand, it is sheltered from the competitive pressures that limit discrimination according to the central prediction of Becker's model. Moreover, public employment is not nearly as sensitive to changes in the business cycle as private employment. Differences in sensitivity to labor market tightness can clearly be observed between the private and public sectors.

Our results partially confirm the hypothesis of a positive link between recession and discrimination as proxied by discrimination in callback rates to job applications. While an unprecedented rise in discrimination on the grounds of both criteria, ethno-racial origin and address, was indeed observed, this only occurred after the lockdown was lifted. Discrimination remained stable throughout the lockdown

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[1] As the term QPV is not necessarily familiar to all employers, we made sure to highlight where the candidate was from by including a building name in his address of residence—names such as "*Immeuble Pavillon*" or "*Tour Nord*" which unambiguously suggests a residence in one of the modern suburban low-rent housing complexes that are synonymous with deprived neighborhoods in France.

and only increased sharply after September 2020 in the last round of correspondence tests conducted. During the lockdown, many jobseekers stopped looking for work which resulted in a surprising drop in the unemployment rate despite the context of a very strong recession: "Many unemployed people have indeed fallen into inactivity[2], in particular because they were unable to carry out active job searches under the usual conditions" (Insee Blog 2020). This drop in applications mitigated the increase in the applicant-to-job ratio that would normally be expected in recessionary conditions. After the lockdown, job openings continued to become scarcer but the number of applications simultaneously rose sharply, exacerbating unemployment and discrimination alike. This result is statistically confirmed when labor market tightness indicators are included in discrimination estimations.

2 Literature review

The cyclical nature of discrimination in the labor market has seldom been studied in the international literature. It is not mentioned in any of the three most frequently cited major literature reviews on discrimination measurement (Riach and Rich 2002; Bertrand and Duflo 2016; Neumark 2018).

The main underlying economic mechanism is simple. In periods of contraction of demand for labor, the number of applicants for each job opening is inversely related to the number of job openings. Employers are faced with an increased influx of applicants for each position advertised. This amplifies the usual workload of recruiters who are attempting to extract the relevant information from the applications received in order to optimize the selection of candidates. These exacerbated difficulties then fuel statistical discrimination which comes from information asymmetry, a situation where employers lack reliable data to enable them to correctly judge the abilities of the candidates. In this context, there is a risk that employers will base decisions on the personal characteristics of applicants, such as their ethnic origin or place of residence, thus leading to a surge in discrimination[3] in access to employment.

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[2] The unemployment halo, which groups together unemployed people who wish to work but who do not meet the ILO criteria for job search or availability to be considered unemployed, increased significantly in 2020. Over the year, it concerned an average of 4.6% of people aged 15–64, an increase of 0.8 percentage points over one year. The increase was particularly marked in the second quarter of 2020, during the first lockdown, with +2.2 points (Insee, French Statistics Institute).

[3] Here, we favor the Arrow-Phelps interpretation of the information discrimination channel. The Becker's preference discrimination channel may also allow us to establish a link between discrimination and the business cycle. In periods of recession when firms' margins are at their lowest, the additional costs borne by discriminating firms should accelerate their failure. According to this mechanism, discrimination should reduce during a recession and be pro-cyclical overall. However, the central prediction of Becker's model that discriminating firms are pushed out of the market in the long run assumes a framework of perfect competition. It is not verified in the more realistic case of imperfect competition, or even in the case where each firm forms a monopsony giving it market power over the candidates seeking recruitment (Manning 2003).

In a situation where there is only one applicant per job opening, discrimination—differential treatment between two candidates—is impossible.[4] In the absence of tightness in recruitment, that is, in a recruitment situation that is perfectly non-selective, there can be no discrimination. Conversely, the likelihood of discrimination is greater as recruitment becomes more selective. If, for example, a recruitment procedure means that there are 1000 candidates for only 10 vacancies, then the task of the recruiters is to choose the 10 best candidates and reject the 990 others. To do so, they will have to use extremely fine and precise information on the abilities of the successful candidates. Hence, there is a risk that they might, at some point in the selection process, extrapolate ability signals from the individual characteristics of the candidates. The higher the number of candidates for a given number of posts, the greater the selectivity in recruitment, the greater the need to screen candidates with similar qualifications, and the greater the risk that personal criteria unrelated to the post will be used to eliminate candidates.

The relationship between selectivity and discrimination has been empirically studied using cross-sectional data. Carlsson et al. (2018) consider three correspondence studies of the Swedish labor market and two different measures of labor market tightness. These two measures produce qualitatively similar results and suggest that ethnic discrimination in hiring decreases during economic downturns, at the opposite of our central hypothesis. Using a correspondence test carried out in Belgium on Turkish minorities, Baert et al. (2015) showed, on the contrary, that while young people whose surname implies foreign origin had the same chances of being called to a job interview as similar profiles with native-sounding names in occupations with low candidates-per-opening ratios, their chances could be halved when the market is tight, i.e., in the most selective screenings. In the same vein, using two self-reporting surveys collecting detailed information about the experience of discrimination in Ireland, Kingston et al. (2015) find no evidence to suggest that discrimination increased during the recession, as reports of discrimination in the workplace have remained relatively stable over time, or declined for some groups, between 2004 (economic boom) and 2010 (economic recession).

In a footnote in his literature review, Neumark (2018, p. 838, footnote 61) emphasizes that a correlation between discrimination and the economic cycle does not necessarily mean that discrimination is sensitive to the business cycle.[5] It may also be related to varying gaps between supply and demand in certain occupations, which would lead to a situation where jobs remain open for longer and there is a lower tendency of employers to discriminate. Another theoretical mechanism involves the effects of the business cycle on the composition of the recruiter pool. During a recession, the most fragile companies disappear. Among these firms, we can assume that recruiters from ethnic minorities and/or located in disadvantaged neighborhoods are disproportionately represented. However, as they are probably the least discriminating, this may increase the proportion of discriminators during a recession. This composition effect may cause discrimination to have a counter-cyclical behavior.

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[4] No discrimination is possible if the offer is a widely publicized open position. There could still be discrimination if the announcement has not been circulated to potential candidates.

[5] This is the only mention of the economic cycle in this comprehensive literature review.

However, the hypothesis of the counter-cyclicality of discrimination has never been validated or invalidated in the temporal dimension so far. To our knowledge, the only exception is the study conducted by Asali et al. (2018) who sent 2200 applications in response to job offers in the Georgian labor market. Their experiment, conducted over a period of twelve months in total, shows a positive correlation between the monthly unemployment rate and the level of discrimination.

Moreover, recruitment selectivity is closely linked to the scarcity of the skills sought. When large firms recruit highly experienced workers, the skills they are looking for are rare and the number of potential candidates for these positions is limited. This type of highly specialized recruitment leaves little room for discrimination. The same is true for qualified occupations for which the market is tight.[6]

The cyclicity of wage discrimination was been much more widely studied, at least since the 1970s (Ashenfelter 1970), but there is no reason to believe that the mechanisms generating differences in pay between employed staff should be the same as those that produce differences in access to employment. Focusing on wage discrimination in the US, Biddle and Hamermesh (2013) found variable outcomes for employees depending on which minority group they belonged to. Relatively, the wage-gap of women and African-Americans with the average widens with negative economic shocks, while the impact on Hispanics is unclear. Their job search model points to two mechanisms with opposite effects that affect these wage differences: changes in the characteristics of the workers in each group over the cycle (composition effects) and changes in pure wage discrimination over the cycle.

3 Background: lockdown and the French labor market

The scale and duration of the Covid health crisis brought about major changes in the French labor market for jobseekers, employees and employers. According to a Unedic survey (Unedic 2020) on these three groups, "two thirds of jobseekers and one third of employees have seen their professional projects impacted." However, there are disparities in these overall outcomes: the impact is greatest on the youngest, the most highly qualified, women and jobseekers. In addition, there are disparities linked to their initial situation in the labor market.

Registrations with the French unemployment agency increased sharply at the very beginning of the first lockdown which occurred on 17 March 2020. The initial shock to the system was huge with an increase of nearly 1,075,000 claimants registered in category A[7] in April, relative to February, a 28.9% increase. The increase was less pronounced when all three categories, A, B, and C, are considered, with a difference between February and April of nearly 365,000 claimants (+6.4%), suggesting a massive shift of jobseekers in reduced activity from categories B and C to category A for claimants without activity. The first effect of the crisis was to sharply

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[6] The risk of discrimination is never nil according to the results of discrimination tests carried out in France for these qualified professions (see for instance Duguet et al. 2013).

[7] Category A claimants are not working at all, while category B claimants worked up to 78 h per month, i.e., more than half time. Category C applicants were employed for more than 78 h per month.

reduce the number of short-term and/or part-time jobs which are common in the restaurant and tourism sectors, sectors particularly hard hit by the crisis. Subsequently, the number of registered workers decreased as the crisis continued to unfold. Over the first year of the health crisis, from February 2020 to January 2021, category A increased by 323,000 (+9.3%) and categories A, B, and C by 340,000 (+6%).

Job seeking methods have evolved with the increased use of digital tools and personal and professional networks. The intensity of job searching differed between the initial lockdown and first ease-of-lockdown period: for 65% of the jobseekers interviewed vs. 80%, respectively, it remained constant or ramped up. Although jobseekers are motivated to change their work situation, to train or to retrain, the return to employment seems to be slower for jobseekers: "*51% consider that they have a medium to high probability of not finding a job, even a short-term contract of less than 6 months, and […] 56% think this is true if they are looking for a longer-term or permanent contract*" (Unedic 2020).

On the other hand, among employers, "*nearly 6 out of 10 establishments indicate that planned recruitment has been postponed or cancelled,*" particularly in sectors that have come to a standstill or are in Paris region. This halting of recruitment had a determining influence on the evolution of unemployment and employment and it continued throughout 2020. Hiring forecasts only started to rise again in 2021, with 41% of employers planning to hire by the end of the year, which is much higher than the recruitment forecasts for 2020. Company size is positively correlated with this upward trend.

4 Data collection

In this study, we rely on a set of five correspondence tests carried out according to a time-invariant protocol. This involves three successive tests carried out 2 years apart before the pandemic: from October 2015 to March 2016, from September 2017 to February 2018, and from October 2019 to February 2020 (Bunel et al. 2016; Challe et al. 2018; L'Horty et al. 2020). We added two new waves, completed during the lockdown just before the summer holidays (from mid-March 2020 to June 2020), and after the lockdown and the summer holidays (in September and October 2020). Overall, we have five sets of data that are comparable in their execution and cover the period from late 2015 to late 2020. This 5 year timespan covers a phase of progressive improvement in the labor market with a drop in unemployment, followed by a brutal recession and a sharp rise in unemployment due to the unexpected health crisis.

The data has both a temporal and a spatial dimension. In the five waves of our correspondence study, the fictitious candidates are located in the eight *départements* that make up the Ile-de-France region[8] and respond to job offers located in their *département*. We made this choice of location in order to limit the distance between

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[8] The eight *departements* of the Ile de France region are: Paris (75), Seine-et-Marne (77), Yvelines (78), Essonne (91), Hauts de Seine (92), Seine-Saint-Denis (93), Val-de-Marne (94), Val d'Oise (95). In Seine-et-Marne, the largest *département* in Ile-de-France, we used two sets of three candidates, one for the north of the *département* and the other for the south.

their homes and the workplaces in the job offers, a factor which is taken into account by employers in Paris region (L'Horty et al. 2019). This allowed us to study the relationships between discrimination and tensions in the labor market which differ, both over time and between *départements*.

We focused specifically on access to a job interview, meaning that no candidates were actually sent to interviews. Neither did the written applications feature a photograph of the candidate. An absence of discrimination at the stage of access to an interview is not proof of an absence of discrimination in the rest of the hiring process. The candidate may still be discriminated against after the interview, but our protocol does not allow us to examine this. However, discrimination during the selection of candidates for interviewing does identify discriminatory practices in the recruitment process.

We focus on an occupation that is of interest to a large number of unemployed people and that provides many job opportunities: administrative and financial managers. Doing so mitigates our chances of being detected as several CVs are sent at the same time and it also reduces the overall number of refusals from employers, independently of any discrimination. Nevertheless, the relatively high success rates of job applicants in an occupation for which the market is tight has a flip side in terms of measuring discrimination: because access to employment is less selective, it may therefore be more difficult to observe discrimination in hiring. We are therefore deliberately placing ourselves in a context where discrimination in the proportion of job applications that result in a call to interview should be below the average. For each period, we compiled all the job offers for administrative and financial manager positions in Ile-de-France we could find by tracking several websites on a daily basis.[9] Once an offer was found, applications were sent on the same day, a few hours apart. These openings were either for limited-term or permanent positions in both the private and public sectors. For each job opening identified, we sent three fictitious applications, all perfectly similar in terms of productive characteristics and individual characteristics except for the one whose effect on access to employment was being tested.

The three fictitious candidates, matched in pairs, only differ in their national origin and the reputation of their place of residence. The first fictitious candidate has a French-sounding first name and surname and lives in a neighborhood with a neutral reputation (reference). The second candidate differs from the reference candidate in that he has a North African sounding first and last name. The third candidate differs from the reference candidate in that he lives in a deprived neighborhood zoned as an "Urban Policy Priority Neighborhood" (QPV).[10] By comparing the reference candidate's chances of success with those of the candidate of North African origin, we can identify if there is any hiring discrimination based on origin. By comparing the success rate of the reference candidate in being offered an interview with that of the candidate residing in the poor neighborhood, we can reveal if there is any discrimination relating to place of residence.[11] All other candidate characteristics are similar.

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[9] These sources include Publidia, Emploi FHF, RDV Emploi public, Emploi collectivité, La Gazette des Communes, Emploi public, Place de l'Emploi Public, Cap Territorial, and Pôle Emploi.

[10] See note 1.

[11] Some examples of locations used in poor neighbourhoods: Boulevard Barbès, 75,018 Paris; Bât Sud, 33 rue de la grande borne 91,350 Grigny; Carreaux 2, 21 Rue Scribe, 95,400 Villiers-le-Bel; Tour Nord, Rue des Hautes Bornes, 94,600 Choisy-le-Roi.

The first names chosen for our candidates of North African origin are, for example, Mehdi, Karim and Mounir and their surnames are Benchargui, Mokraoui and Mehdaoui. The first names of our candidates of French origin are Nicolas, Julien and Guillaume and their surnames are Mercier, Legrand, Lambert and Blanc. These names were chosen following a proven procedure using the exhaustive historical files of names and surnames from the general census of the population, published by the French national institute of statistics, INSEE. We chose from the ten most frequent names and surnames in the year of birth of the candidates. The names indicating a North African origin were selected according to their date of entry in the file which corresponds to the colonial period of French North Africa, from 1830 to 1952. These names indicate above all the geographical origin of the candidate but there is also an association with religion, since the majority of French people originating from North Africa are Muslim. They also act as a proxy for ethnic and cultural differences. All these differences are encompassed in the notion of discrimination based on origin.

The candidates are men in their thirties, of French nationality, single and without children. They state that they are mobile (they have a driver's license and their own vehicle). They have a master's degree in accounting, audit and control and they indicate similar career paths with no period of unemployment. For job openings in the public sector, the candidates mention in their applications that they have passed the "*territorial attaché*" exam the previous year, which allows them to apply for positions in the local civil service.

It bears noting that the same application material was used for all five waves of data collection. To avoid the style or content of a particular application having a systematic influence on the selection of a particular candidate (despite the precautions taken when constructing the applications), we randomly swapped CVs and cover letters between the fictitious candidates. Application material was thus alternated between the candidates throughout the data collection process. Applications in response to any given job offer were sent out by e-mail over two consecutive days from each candidate's mailbox as soon as the offer was posted on a job website. The order in which applications were sent for any given vacancy was selected randomly so that, over all the vacancies tested, the application of each fictitious candidate was sent the same number of times in each position.

Responses to the applications were considered positive when the recruiter either invited the fictitious candidate to an interview or contacted him by e-mail or telephone to obtain more information about his current situation or qualifications. Conversely, responses were considered negative when the recruiter formally rejected the application or did not reply before the end of the collection period. The composition of the sample of job openings tested is presented in Table 1. Overall, the sample includes 1583 job offers covering both the public and private sectors. This means that 4749 (resp. 609) applications were sent out (3×1583 postings). Around two-thirds of the offers were in the private sector. The first three waves of correspondence tests included 554 job offers (2015–2016), 384 job offers (2017–2018) and 248 job offers (2019–2020). A total of 194 job postings were tested during lockdown and 203 afterward between September and October 2020. The last test wave is shorter than other waves because it corresponds to the period between the two lockdowns.

Table 1 Number of administrative manager job offers

Period	Total	Private	Public	Private/total (in %)
Wave 1 (October 2015—> March 2016)	554	393	161	70.9
Wave 2 (September 2017—> February 2018)	384	222	162	57.8
Wave 3 (October 2019—> February 2020)	248	187	61	75.4
Wave 4 (March 2020—> June 2020)	194	130	64	67.0
Wave 5 (September 2020—> October 2020)	203	134	69	66.0
Number of postings	1583	1066	517	67.3

Source: RED correspondence test (TEPP-CNRS), authors' calculations

Our results should nonetheless be considered with caution insofar as correspondence testing does not provide an exhaustive measure of hiring discrimination. It only indicates that it exists. Our repeated tests enable us to detect discrimination in selection for interview, thus highlighting the existence of discriminatory behavior in the recruitment process. However, the absence of discrimination during this stage does not prove that the rest of the hiring process is free of discrimination. The candidate may very well be discriminated against after the interview but our protocol does not enable us to measure this possibility. Thus, our estimates may be considered to be at the lower limit for the presence of discrimination in recruitment in France.

Another concern is that the sample size of the five waves varies over time. This has an impact on our ability to detect potential discrimination, as the power is lower with smaller sample sizes. Suppose for instance that the response rate for a reference candidate is 0.2 and that the proportion is five percentage points lower (0.15) for a North African candidate. In such a setting, we calculate the power corresponding to various sample sizes ranging between 200 (approximately our sample size in wave 4 and wave 5) and 550 (our sample size in wave 1). The calculations show a concave relationship between power and sample size. The power is about 0.862 for $n=550$, 0.727 for $n=400$, and 0.415 for $n=200$. At the same time, both the response rate of the reference candidate and possibly the magnitude of the discrimination may vary over time. Whatsoever, it should be kept in mind that we are less likely to detect discrimination in the last three waves due to reduced sample sizes.

5 Results

5.1 Descriptive statistics on callback rates

For the five waves, we calculated the callback rates for the three candidate profiles,[12] both overall and by sector (private vs. public). The corresponding rates are reported in Table 2.

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[12] Sending multiple applications to the same firm could drive induced competition (see Larsen 2020). We are not able to take this potential effect into account as we do not observe the other applications sent to the offers we responded to with our fictitious candidates.

Table 2 Callback rates (in %) by profile

Profile	All			Private			Public		
	(1A) French-neutral	(2A) North African – neutral	(3A) French-poor	(1B) French-neutral	(2B) North African-neutral	(3B) French-poor	(1C) French-neutral	(2C) North African-neutral	(3C) French-poor
Wave 1 (10/2015–03/2016)									
Callback rate (in %)	14.4	11.2	12.6	7.6	6.6	8.1	31.1	22.4	23.6
Comparison to reference		−3.2**	−1.8		−1.0	0.5		−8.7**	−7.5**
p value		0.018	0.189		0.434	0.716		0.013	0.023
Wave 2 (10/2017—> 11/2018)									
Callback rate (in %)	18.2	16.4	19.3	10.8	9.9	13.1	28.4	25.3	27.8
Comparison to reference		−1.8	1.1		−0.9	2.3		−3.1	−0.6
p value		0.250	0.538		0.618	0.354		0.277	0.783
Wave 3 (10/2019—> 02/2020)									
Callback rate (in %)	22.2	18.1	26.2	20.9	13.9	25.1	26.2	31.1	29.5
Comparison to reference		−4.1	4.0*		−7.0**	4.2		4.9	3.3
p value		0.132	0.086		0.023	0.117		0.370	0.484
Wave 4 (03/2020—> 06/2020)									
Callback rate (in %)	23.2	19.6	22.2	19.2	14.6	16.9	31.3	29.7	32.8
Comparison to reference		−3.6	−1.0		−4.6	−2.3		−1.6	1.5
p value		0.179	0.696		0.134	0.441		0.766	0.766
Wave 5 (09/2020—> 10/2020)									
Callback rate (in %)	28.6	15.8	22.2	28.4	11.2	19.4	29.0	24.6	27.5
Comparison to reference		−12.8***	−6.4***		−17.2***	−9.0***		−4.4	−1.5
P value		0.000	0.006		0.000	0.004		0.321	0.658

Source: RED correspondence test (TEPP-CNRS), authors' calculations

Significance levels are 1% (***), 5% (**) and 10% (*)

First of all, it is interesting to observe that the reference candidate's callback rate increased from 14.4% in 2016 to 22.2% at the beginning of 2020 (column 1A, Table 2). This increase reflects the gradual improvement of the French labor market over this period. Between 2016 and the start of the Covid pandemic, all labor market indicators had gradually improved in France. The unemployment rate had fallen from 10.0% in the fourth quarter of 2015 to 7.9% in the first quarter of 2020. The number of salaried jobs had gradually increased until it reached a peak of 25.5 million in the private sector, an increase of more than 780,000 jobs over the period (+3.2 pp).

A second interesting finding relates to the difference in callback rates between private and public sector jobs (columns 1B and 1C, Table 2). In all of the waves, the callback rates are lower for the private sector than for the public sector with a narrowing gap over time. The difference between the two sectors was at its peak at the beginning of the period (+23.5 pp) and practically nil by the end (+0.6 pp). This convergence seems to be driven by a change in callback rates in the private sector. The reference candidate's success rate remained stable at a relatively high level for the public sector (ranging between 26 and 31%), while it gradually increased for the private sector. These developments are fully in line with employment trends in the economy with improvements solely in the private sector (Passeron 2021). Between the end of 2016 and the end of 2019, employment increased by 782,700 in the private sector compared with a decline of 4,000 in the public sector over the same period.

Taking this as the benchmark rate, the candidate of North African origin almost systematically had lower success rates, although the differences were not always significant. At the beginning of the period (end of 2015 and beginning of 2016), the differences in callback rates were only significant in the public sector. While the reference candidate was contacted in 31.1% of cases, the success rate of the candidate of North African origin was 8.7 pp lower. The candidate of French origin living in a poor neighborhood had a success rate that was 7.5 pp lower. Both differences are significant at the 5% level, corresponding to a confirmation of discrimination in selection for interview against applicants of North African origin and applicants living in a poor neighborhood.

During the next rounds of testing (waves 2 and 3), the success rates of every candidates rose in the private sector and there were very few significant differences in callback rates. The only exceptions are in 2019 (wave 3). There was discrimination against the North African candidate in access to private sector employment, with a difference of 7 points less than for the reference candidate. On the other hand, there was a positive gap for the candidate living in a poor neighborhood when the private and public sectors were combined. This gap may be due to the effects of the "*Emplois Francs*" scheme implemented in France in 2018, an initiative which gave a subsidy to employers when they hired a jobseeker living in a QPV (Challe et al. 2020). However, this gap is not significant for either the private or public sector when sector-specific differences are estimated.

Wave 4 corresponds to the lockdown period which began on March 17 in France. Overall, the callback rate of the reference candidate increased by one percentage point (from 22.2 to 23.2 pp). However, the context was one of strong recession with an annualized fall in economic activity estimated at 8.3 pp by

INSEE, the French National Statistics Institute. The reference candidate's success rate slightly decreased in the private sector (from 20.9 to 19.2 pp), yet increased in the public sector (from 26.2 to 31.3 pp).

This should undoubtedly be interpreted in the same way as the evolution of the unemployment rate, which fell over the same period, from 7.9% in the first quarter of 2020 to 7.1% in the second quarter. According to INSEE (see Passeron 2021), the lockdown not only brought about a drop in job openings, but also caused many unemployed people to stop searching for a job. In our field experiment, it is likely that the fictitious candidates were facing less competition from real candidates (not monitored in this test), which could explain their improved callback rate during the lockdown period, particularly in the public sector where job offers remained stable. Stable and low discrimination in access to employment appears to be confirmed during the lockdown. In any case, we did not detect any ethnic or residential discrimination in wave 4 in either the private or public sector.

The situation completely changed in wave 5 which occurred after the first lockdown but before the second starting on October 29 in France. During this period of "freedom from lockdown", the success rate of the reference candidate showed a strong increase from 23.2 to 28.6% ($+5.6$ pp) for all job offers. Again, this increase reflects shifts occurring in the private sector as the success rate in the public sector dropped by 2.3 pp (from 31.3 to 29.0 pp). During this period, there was a strong increase in GDP growth. However, the increase in callback rates in private employment was not reproduced for the North African candidate and there was very little effect on the candidate from a poor neighborhood. This meant that the gaps were considerably widened, indicating discrimination that is statistically significant at the 1% level. The penalty for the North African applicant was 17.2 pp relative to the reference applicant, and that of the applicant living in a poor neighborhood was 9 pp (columns 2B and 3B, Table 2). Conversely, there was no significant difference in callback rates between the various candidates in the public sector.

There was a sharp increase in unemployment following the first lockdown with the unemployment rate inflating from 7.1 to 9% in a single quarter in France. Jobseekers, newly released from lockdown, resumed their search. While employment recovered slightly under the effect of the upturn in economic activity, the number of applicants per job opening increased much more strongly, thus exacerbating unemployment. In these new circumstances, discrimination appears to have increased at an unprecedented rate, but only in the private sector.

We delved further to examine how employers discriminated between the candidates by considering three possible outcomes for each job offer: no candidates were contacted; only some candidates were contacted; all candidates were contacted. The continual improvement in the employment situation between waves 1 and 3 was accompanied by a decline over time in the number of cases where no candidate received any callback (from 78.9% in 2015–2016 to 66.5% in 2019–2020). Simultaneously, there was an increase first in the proportion of the job offers tested where all three candidates received a callback (in 2017–2018), and then, where only some candidates received a callback (2019–2020). While there was little change during

the lockdown, we did note an increase (+ 1.5 pp) in the proportion of job offers for which all candidates received a callback in wave 5.

5.1.1 Econometric analysis

In our experiment, the characteristics of all the candidates are similar with one exception: applications vary by the candidate's national origin or by the reputation of his place of residence. However, there was still some heterogeneity in the various applications sent to recruiters (the order in which they were sent and the documents used to submit candidate-specific curriculum vitae, cover letters, etc.) as well as in the employer characteristics (location of the job, firm size,[13] etc.) for the various job offers. We turn to econometric analysis to control for these various characteristics. Let R_{ji} be the response sent by employer j to the candidate i such that $R_{ji} = 1$ when the candidate is contacted and $R_{ji} = 0$ otherwise. We estimate linear probability models to explain the probability of callback with standard errors clustered at the job level[14]:

$$R_{ji} = \sum_k \delta_k * c_{ki} + \sum_t \mu_t * \text{wave}_t + X_{ji}\beta + \varepsilon_{ji} \tag{1}$$

where $\rfloor_{ki}$ refers to the type of candidate ($k = 1$ for North African origin, $k = 2$ for living in a poor neighborhood, and $k = 0$ for the reference candidate), wave_t is a dummy variable corresponding to the test wave, X_{ji} is a set of control variables, and δ_k, μ_t and β are parameters to be estimated, and ε_{ji} is a residual with $E(\varepsilon_{ji}) = 0$ The selected covariates include: the immediate availability of the candidate if required in the job offer; whether the job offer is in the public sector; the *department* in which the position is located; the gender of the recruiter; the order of sending; the formatting of the application. Figure 1 shows the selectivity of employers among the three profiles. A large proportion of employers do not respond to any of the fictitious candidates. We are interested in those who respond to only some candidates to identify differences in treatment.

The corresponding estimates are shown in panel A of Table 3. When both sectors are combined (column 1), we find that the North African candidate has a lower probability of callback than the reference candidate, with −4.3 pp ($t=−4.78$). The marginal effect of the private (−4.5 pp, $t=−4.46$) and public (−4.0 pp, $t=−2.23$) sectors is very comparable. Conversely, the reputation of residential location has no effect. As discussed earlier, there are substantial changes over time in the probability of being contacted. Compared to wave 1, the probability of callback is 11.2 pp ($t=3.00$) higher in wave 3 and 10.4 pp ($t=3.12$) higher in wave 5 while there is no significant difference between waves 1, 2 and 4.

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[13] Size of the company is not a covariate used in our model because of the difficulty in finding this information, especially for small companies. We suspect there may be large missing values for this variable. Moreover, Baert et al. (2018) find no evidence of an association between firm size and hiring discrimination. On the other hand, the authors do find suggestive evidence that hiring discrimination is lower in public and non-profit firms (compared to commercial firms).

[14] We have also estimated linear probability models and get very similar results.
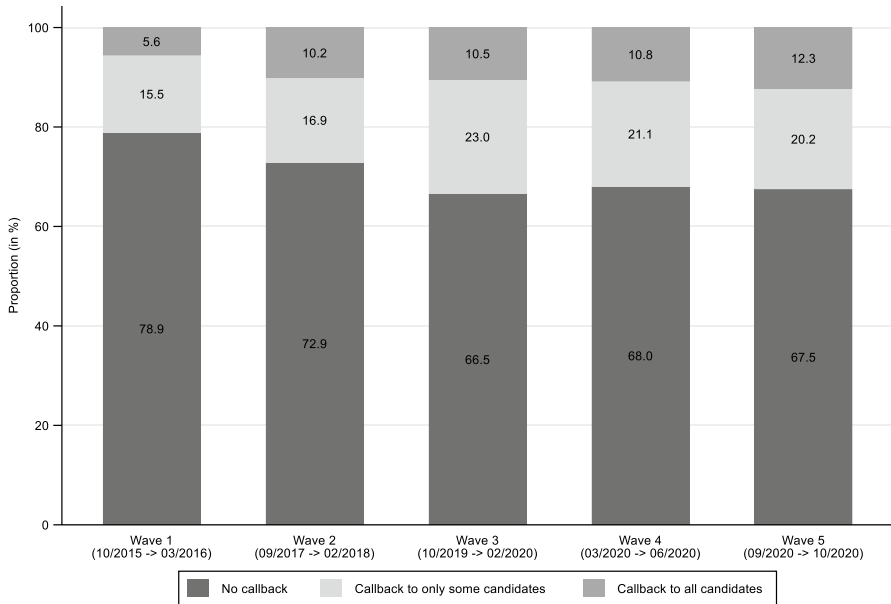
Fig. 1 Pattern of callback rates over time. *Source*: RED correspondence test (TEPP-CNRS), authors' calculations

To study whether discrimination varies over time, we extend (1) by adding a set of interaction terms that cross the wave dummies with the various candidates:

$$R_{ji} = \sum_{k} \sum_{t} \mu_{kt} * c_{ki} * \text{wave}_t + X_{ji}\beta + \varepsilon_{ji} \qquad (2)$$

where the coefficients μ_{kt} measure the effect of a specific profile at a given date. Note that we no longer include the dummy variables wave_t in (2) as they are always interacted with the candidate profile. The reference category is the French-neutral candidate interacted with the first wave. As a consequence, this means that for a given wave t, for example, the effect of being the North African candidate versus reference candidate is measured by the difference $\mu_{1t} - \mu_{0t}$. (and $\mu_{2t} - \mu_{0t}$ measures any discrimination between the poor candidate and the reference candidate). The corresponding estimates are reported in panel B of Table 3. To make the presentation easier, we plot the difference in callback rates for origin and poor residential address in Fig. 2.

Very little discrimination in access to employment was observed before the lockdown. Taking both the private and public sectors, it was only found in the first wave for the criterion of ethnic origin (with -3.2 pp, $t=-2.39$). In fact, there are differences between the two sectors as discrimination is only observed in the public sector: -8.7 pp ($t=-2.50$) for origin and -7.4 pp ($t=-2.27$) for poor residential address. In the third wave (2019), discrimination against applicants of North African origin was detected, but only in the private sector (-6.6 pp, $t=-2.18$). Reverse

Table 3 Linear probability estimates of callback

A. Without wave interaction terms

Variables	(1) All		(2) Private		(3) Public	
North African–neutral	−0.043***	(−4.78)	−0.045***	(−4.43)	−0.040**	(−2.23)
French–poor	−0.007	(−0.82)	−0.000	(−0.01)	−0.023	(−1.43)
Public sector	0.094***	(4.23)				
Wave fixed effects	YES		YES		YES	
Control variables	YES		YES		YES	
Number of observations	4749		3198		1551	
R²	0.076		0.050		0.072	
B. With wave interaction terms						
FrenchxWave 1 (10/2015->03/2016)	REF		REF		REF	
FrenchxWave 2 (10/2017->11/2018)	−0.013	(−0.46)	0.029	(0.93)	−0.154*	(−1.90)
French x Wave 3 (10/2019->02/2020)	0.095**	(2.28)	0.142***	(3.06)	−0.053	(−0.52)
French x Wave 4 (03/2020->06/2020)	0.034	(0.86)	0.066	(1.57)	−0.087	(−0.93)
French x Wave 5 (09/2020->10/2020)	0.151***	(3.78)	0.225***	(4.93)	−0.068	(−0.73)
North African x Wave 1 (10/2015->03/2016)	−0.033**	(−2.39)	−0.010	(−0.80)	−0.087**	(−2.50)
North African x Wave 2 (10/2017->11/2018)	−0.032	(−1.11)	0.014	(0.48)	−0.190**	(−2.28)
North African x Wave 3 (10/2019->02/2020)	0.053	(1.38)	0.076*	(1.84)	−0.007	(−0.07)
North African x Wave 4 (03/2020->06/2020)	0.000	(0.01)	0.023	(0.62)	−0.099	(−1.08)
North African x Wave 5 (09/2020->10/2020)	0.024	(0.70)	0.055	(1.53)	−0.109	(−1.20)
Poor x Wave 1 (10/2015->03/2016)	−0.018	(−1.33)	0.005	(0.36)	−0.074**	(−2.27)
Poor x Wave 2 (10/2017->11/2018)	−0.004	(−0.14)	0.048	(1.45)	−0.164**	(−1.99)
Poor x Wave 3 (10/2019->02/2020)	0.136***	(3.28)	0.189***	(4.13)	−0.022	(−0.21)
Poor x Wave 4 (03/2020->06/2020)	0.025	(0.66)	0.042	(1.03)	−0.066	(−0.73)
Poor x Wave 5 (09/2020->10/2020)	0.087**	(2.30)	0.136***	(3.21)	−0.088	(−0.94)
Public sector	0.094***	(4.23)				
Control variables	YES		YES		YES	
Number of observations	4749		3198		1551	
R²	0.078		0.055		0.074	

Source: RED correspondence test (TEPP-CNRS), authors' calculations

Estimates from linear probability models with standard errors clustered at the job offer level. Significance levels are 1%(***), 5%(**) and 10%(*). Control variables include a dummy indicating whether the job is to be filled within two months; the department in which the post is located; the gender of the recruiter; the order of sending of the applications; the formatting of documents sent

discrimination for place of residence was also detected in the private sector at the 10% level (+4.7 pp, $t=1.70$), which may correspond to an impact from the "*Emplois Francs*"[15] scheme. During lockdown, no discrimination was detected on the grounds of either origin or address. Finally, after the lockdown, statistically significant discrimination, both ethnic and residential, appeared in the private sector only. The

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[15] "*Emplois Francs*" (EF) is a new public policy in France implemented since April 2018 in nearly 200 districts that provides financial assistance to companies when they hire a jobseeker living in a disadvantaged neighborhood (named QPV for "*Quartier Prioritaire de la Ville*"). After an initial experimental phase, the "*Emplois Francs*" scheme has been extended to all QPVs since 1st January 2020. The name of the policy has no direct translation in English.

marginal effects are large, with -17.0 pp for origin ($t=-4.54$) and -8.8 pp for notoriety of address ($t=-2.85$).

5.1.2 Robustness checks

A first drawback of the estimates reported in Table 3 is that they do not take account of all any heterogeneity at the employer level. For instance, we do not control for firm size or composition of the workforce (in terms of ethnicity or social status). Such firm characteristics could affect the coefficients δ_k or μ_{kt}. As the correspondence test consists of sending three applications for each job offer, we can estimate a version of our previous regressions to account for such unobserved heterogeneity. We add an employer specific-component effect θ_j in Eq. (1):

$$R_{ji} = \sum_k \delta_k * c_{ki} + \sum_t \mu_t * wave_t + X_{ji}\beta + \theta_j + \varepsilon_{ji} \tag{3}$$

We turn to a fixed effect linear probability model. In this context, any potential discrimination effect is identified from situations where some candidates, but not all, receive a positive answer. By construction, all observable characteristics which are invariable at the job offer level are excluded from the list of covariates.

The corresponding results are displayed in Table 4. They are very similar to our previous findings, so we can conclude that unobserved heterogeneity at the job offer level is not a major concern. Our main results are twofold. First, when all waves are combined, we find some discrimination against North African candidates (-4.3 pp and $t=-4.79$ in both the private and public sectors) but living in a poor neighborhood does not reduce the probability of receiving a callback compared to the reference candidate. Second, in the period after lockdown was lifted, we find a sharp increase in discrimination on the grounds of both ethnic origin (-17.0 pp and $t=-4.55$) and reputation of place of residence (-8.8 pp and $t=-2.86$) in the private sector only.

A second potential limitation of our analysis is that we did not test for a direct effect of labor market tightness on discrimination. The spatial dimension of our data allows us to take this assessment a step further in that direction. For each of the five waves of tests, we located sets of three fictitious candidates in each of the 8 *departments* of the Ile-de-France region. This enabled us to construct departmental indicators of labor market tightness for each test period. We calculated the departmental tightness rate by dividing the average number of jobseekers registered with *Pôle Emploi*[16] during each test period into the number of job offers posted by *Pôle Emploi*. The results are shown in the second part of Table 4. In the private sector, the penalty suffered by applicants of North African origin is mitigated by taking account of the effects of labor market tightness which prove to be statistically significant when crossed with North African origin. When the number of candidates

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[16] *Pôle Emploi* is the French national employment agency. We considered all jobseekers in categories A, B, and C to correspond to registered jobseekers whether or not they are in reduced activity, as long as they are immediately available to take up employment.
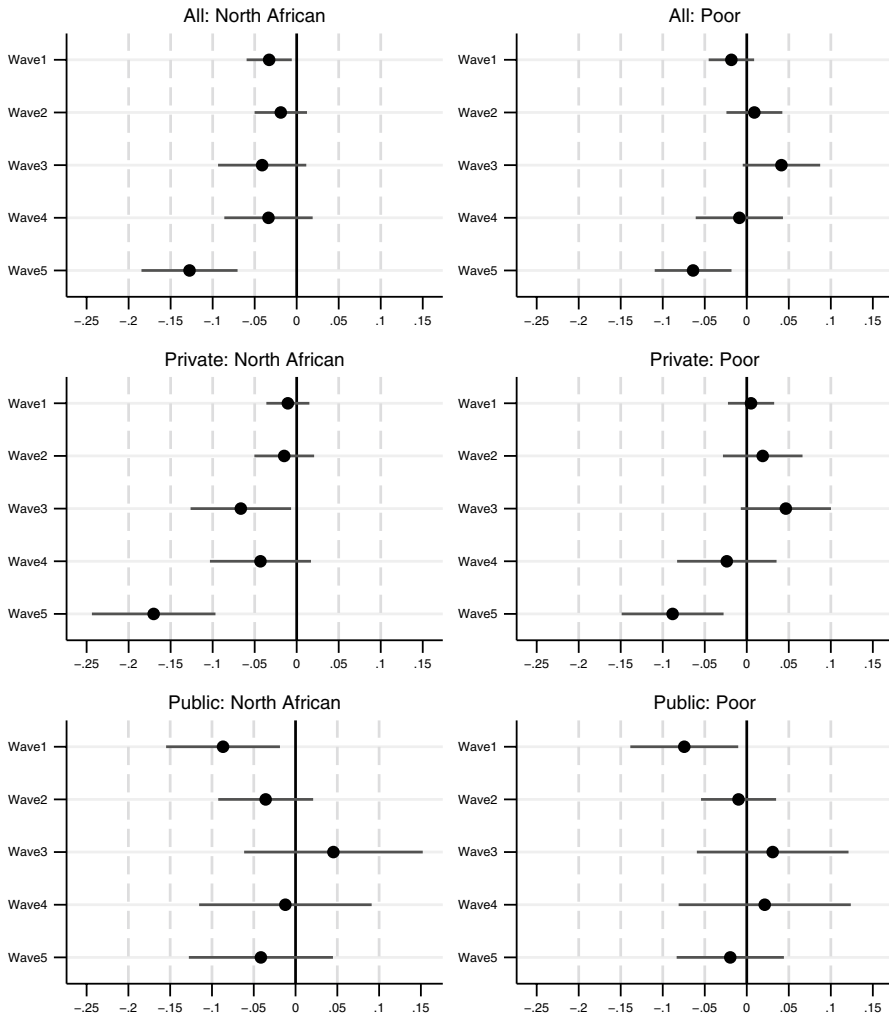
Fig. 2 Differential in callback rates based on origin and poor residential address. *Source*: RED correspondence test (TEPP-CNRS), authors' calculations

per job offer increases, the discrimination suffered by candidates of North African origin also increases and is statistically significant.

A third potential limitation relates to multiple-hypothesis testing given the design of our experiment. Specifically, there are two candidate profiles being tested with five outcomes corresponding to the receipt of callbacks for each wave. There are thus 20 hypothesis tests with 20 distinct *p* values. Assuming that all null hypotheses are true and that the outcomes are independent, then the probability of having at least one false rejection with a critical value of 5% would be 40.1%. Several methods have been proposed to reduce the possibility of false rejections.

Table 4 Fixed effect linear probability estimates of callback

Variables	(1) All		(2) Private		(3) Public	
North African–neutral	−0.043***	(−4.78)	−0.045***	(−4.43)	−0.040**	(−2.23)
French–poor	−0.007	(−0.82)	−0.000	(−0.01)	−0.023	(−1.43)
Labor market tightness(log)	0.124	(1.18)	0.043	(0.37)	0.304	(1.30)
Public sector	0.094***	(4.24)				
Wave fixed effects	YES		YES		YES	
Control variables	YES		YES		YES	
Number of observations	4749		3198		1551	
R²	0.076		0.050		0.074	
Variables	(1)All		(2)Private		(3)Public	
North African–neutral	−0.034***	(−3.55)	−0.032***	(−3.17)	−0.043*	(−1.91)
French–poor	−0.000	(−0.03)	0.013	(0.85)	−0.037	(−1.26)
Labor market tightness(log)	0.133	(1.27)	0.059	(0.51)	0.299	(1.27)
North African–neutral x Labor market tightness(log)	−0.023*	(−1.67)	−0.037**	(−2.37)	0.005	(0.19)
French–poor x Labor market tightness(log)	−0.003	(−0.56)	−0.008	(−1.04)	0.007	(0.57)
Public sector	0.094***	(4.24)				
Wave fixed effects	YES		YES		YES	
Control variables	YES		YES		YES	
Number of observations	4749		3198		1551	
R²	0.077		0.051		0.074	

Source: RED correspondence test (TEPP-CNRS), authors' calculations

Estimates from fixed effect linear probability models. Significance levels are 1%(***), 5%(**) and 10%(*). Control variables include a dummy indicating the random order in which applications are sent

A first approach is to control the Familywise Error Rate Control (FWER). Considering a set of m tested hypotheses, of which n are true, FWER is the probability that at least one of these true hypotheses is rejected. The more hypotheses there are, the greater the FWER, as the probability of rejecting at least one hypothesis increases. Using FWER techniques, p values are adjusted to reduce the probability of a false rejection. A second approach is to calculate the sharpened False Discovery Rate (FDR) q-values (Anderson, 2008). Contrary to FWER, FDR takes account of the trade-off between correct and false rejections. We denote the number of false rejections as FR, the number of correct rejections as CR, and the total number of rejections as $T = FR + CR$. While FWER is the probability $Pr(FR > 0)$, FDR is the expected proportion of all rejections having type I error, i.e., $FDR = E[FR/FR + CR]$. When all null hypotheses are true, then FDR = FWER. However, in the general case when some false hypotheses are incorrectly rejected, then FDR will be lower than FWER. So, FDR requires lower p value adjustments.

We applied both correction techniques to our dataset. Concerning FWER, given that the classical Bonferroni correction suffers from a power issue, we rely on the free step-down resampling method (with 10,000 bootstrap draws) proposed by Westfall and Young (1993) and further discussed in Damon et al. (2019). For FDR,

we rely on the two-stage procedure described in Benjamini et al. ([2006]). In both cases, we report corrected p values and q-values based on models including covariates with standard errors clustered at the job offer level. Our results are shown in Table [5]. Keeping in mind the limitation of our sample size, we note that both the FDR q-values and FWER p values are substantially higher than the typical p values. However, our main finding that there is a high level of discrimination on the grounds of both origin and poor residential address in the private sector after lockdown was lifted remains valid. In all cases, the corrected q- and p values are significant at the 5% level.

6 Concluding comments

In this paper, we investigated the evolution of discrimination in access to employment (proxied by callbacks following job applications) over time in France using an innovative set of repeated correspondence tests. The methodology design covers only one type of job, that of administrative manager, both in the private and public sectors, and two discrimination criteria, ethnic origin and reputation of place of residence. As the correspondence tests were implemented following the same methodology each time, we were able to track the evolution of discrimination over time since 2015 through waves of tests conducted before, during and after the Covid lockdown.

Our results indicate that hiring discrimination based on the grounds of origin and place of residence has tended to decrease in France since the mid-2010s, in the context of an improved labor market, but that it increased sharply during the Covid health crisis, this time in a context of recession, suggesting an overall counter-cyclical nature of discrimination. There was a sharp increase in discrimination after the first lockdown, on the grounds of both origin and place of residence. This increase only occurred in the private sector, thus confirming our theory that tightness in the labor market is the main explanation for this evolution in discrimination. This interpretation is confirmed by a direct test conducted by adding an indicator of labor market tightness to an explanatory model of discrimination. An increase in the number of applicants per job is positively associated with an increase in discrimination.

However, we cannot rule out the possibility that other mechanisms are also at work that would help explain the sharp rise in discrimination after the first lockdown. One explanation proposed is that the increased discrimination against candidates from disadvantaged neighborhoods could also be explained, at least in part, by employers' desire to protect their companies from sources of Covid contamination. Indeed, disadvantaged neighborhoods were particularly exposed to the virus. During the pandemic, people from these neighborhoods were doubly penalized: they were more likely to be made unemployed because their jobs were more likely to be lost (low-skilled jobs, "non-essential" sectors such as restaurants). Moreover, for those who were able to keep their jobs (essential sectors such as commerce, personal services, delivery, cleaning), they were much less likely to be able to work remotely, meaning that they were more exposed to the virus. Another reason for their higher level of exposure to contamination is the high population density in these areas (Sala [2018]). A high proportion of housing in priority neighborhoods is overcrowded

Table 5 Results from multiple-hypothesis testing

Profile	All		Private		Public	
	(1A)	(1B)	(2A)	(2B)	(3A)	(3B)
	North African–neutral	French-poor	North African-neutral	French-neutral	North African-neutral	French-poor
Wave 1 (10/2015–03/2016)						
Treatment	−0.032	−0.018	−0.010	0.005	−0.086	−0.074
p value	0.019	0.193	0.439	0.718	0.014	0.026
FDR sharpened q-value	0.054	0.236	0.540	0.683	0.161	0.161
FWER Westfall-Young p value	0.127	0.631	0.895	0.895	0.108	0.177
Wave 2 (10/2017—> 11/2018)						
Treatment	−0.020	0.008	−0.018	0.017	−0.034	−0.014
p value	0.221	0.652	0.323	0.466	0.258	0.560
FDR sharpened q-value	0.236	0.465	0.480	0.540	1.000	1.000
FWER p value	0.631	0.877	0.845	0.895	0.875	0.979
Wave 3 (10/2019—> 02/2020)						
Treatment	−0.041	0.044	−0.064	0.052	0.042	0.028
p value	0.133	0.068	0.042	0.071	0.461	0.552
FDR sharpened q-value	0.211	0.139	0.132	0.169	1.000	1.000
FWER p value	0.552	0.362	0.270	0.382	0.968	0.979
Wave 4 (03/2020—> 06/2020)						
Treatment	−0.031	−0.008	−0.041	−0.019	−0.013	0.016
p value	0.262	0.786	0.200	0.564	0.807	0.761
FDR sharpened q-value	0.246	0.465	0.322	0.604	1.000	1.000
FWER p value	0.631	0.877	0.707	0.895	0.979	0.979
Wave 5 (09/2020—> 10/2020)						
Treatment	−0.129	−0.064	−0.171	−0.091	−0.039	−0.012
p value	0.000	0.006	0.000	0.004	0.395	0.712
FDR sharpened q-value	0.001	0.032	0.001	0.024	1.000	1.000
FWER p value	0.000	0.049	0.000	0.035	0.953	0.979

Source: RED correspondence test (TEPP-CNRS), authors' calculations

P value is obtained from a linear regression model with standard errors clustered at the job offer level, FDR(false discovery rate) sharpened q-value is calculated following Anderson(2008), FWER(family wise error rate) p value is calculated using the step-down resampling methodology described in Westfall and Young(1993) with 10,000 bootstraps

(22% of households in priority neighborhoods compared to 12% outside priority neighborhoods in France as a whole, and 33% and 20%, respectively, for the Île-de-France region, i.e., the highest proportion with the exception of overseas territories). Severe overcrowding affects 4% of households in priority neighborhoods, compared with 1% outside them. The media coverage of cases of non-compliance with sanitary rules in these neighborhoods also tarnished their image (Noûs 2020). All of these elements may have contributed to employer fears that residents of disadvantaged neighborhoods could be more affected by the virus and more contagious. This

would be a reason for discrimination against people living in the most disadvantaged neighborhoods, even against qualified people applying for jobs such as administrative manager that had disappeared to suddenly reappear. From this point of view, it is interesting in retrospect to have chosen a profession requiring a high level of competence to test, even if it is worth carrying out tests on other professions in order to verify the general nature of our results.

The increase in discrimination means that the populations most impacted by the effects of the economic crisis associated with the lockdown are penalized twice. It amplifies the inequalities already caused by the crisis. This is a particularly worrisome finding that requires a public policy response. In addition to the economic recovery initiatives and employment support actions implemented by the State to cushion the effects of the crisis, our findings suggest that specific actions to combat discrimination should also be considered. These actions could involve sending reminders of employers' legal obligations to not discriminate; increasing sanctions; raising awareness of the problem; training recruiters on the issue of discrimination.

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